Letters to the Editor.

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Chemical Warfare.

SIR EDWARD THORPE, in his review of Victor Lefebure's book, "The Riddle of the Rhine," in NATURE of November 10, p. 331, quotes a passage which deals with my own work during the initial stages of the war, and that of the Kaiser Wilhelm-Institut für Physikalische Chemie, of which I am the principal. The intention is to make the world believe that the materials for gas warfare were prepared by the German military authorities and chemical industry for the intended war, and that experiments with this end were carried out in my institution, if not previous to the war, at least from August, 1914, onwards.

It is always dangerous to attempt to form a correct estimate of the intentions of others from the traces of events they have left behind them. But the greatest errors must necessarily arise if an outsider tries to deduce from his own impressions the intentions of men whose ways of thinking he does not know and cannot understand.

Perhaps there might have been some ground for suspicion if Germany could have foreseen the trench warfare, and if we could have imagined that the German troops could ever be held up for weeks and months before the enemy's wire entanglements. But previous to the war, and up to the Battle of the Marne, everyone in Germany imagined that the course of the war would be a succession of rapid marches and great pitched battles, and what use would gas have been to a field army in such a war of movements? I think I may safely say that during the course of the war I became acquainted with every man of any importance in the army, in industry, and in science, who had anything to do with chemistry as applied to military offensive and defensive operations, and that I am well informed regarding the development and the course of chemical warfare. Yet among all these men I have never met one who, previous to the war or during the first two months of its course, had conceived the idea of providing the field army with gas, or had made experiments or preparations for such a purpose. We had actually first to read in the French, Italian, and English Press—as, for instance, in the *Pall Mall Gazette* of September 17, 1914—of the terrible things that were in preparation for us before we began to make similar preparations in view of the commencement of the war of position.

As regards my own institution and its work during the first months of the war, that intelligent person who, according to the passage in Lefebure quoted by Sir Edward Thorpe, observed my activities in my institute from behind a wall, lacked the gift of interpreting correctly what he saw and heard. Visitors in grey Headquarters motors did indeed come to my institution in August, 1914, though not to see me upon the subject of chemical means of warfare, but because Headquarters were very anxious to know how motor spirit could be made proof against the cold of a Russian winter without the addition of toluol. The question of gas as means of warfare did not begin to engage our attention until the first three months of war had passed.

In war men think otherwise than they do in peace, and many a German during the stress of war may have adopted the English maxim, "My country, right or wrong," but that German science and industry before the war made preparations with deliberate intent for gas warfare against other nations is an assertion that, in the interest of the necessary interdependence of the nations in the realms of science and industry, must not be allowed to go uncontradicted in so serious and respected a journal as NATURE.

F. HABER.

Kaiser Wilhelm-Institut, Berlin-Dahlem,
December 17.

HERR GEHEIMRAT HABER takes exception to the quotation I made from Major Lefebure's "Riddle of the Rhine," in the course of my notice of that book, on the ground that it implies that the German military authorities were prepared to ignore their undertaking, under the Hague Convention, to abstain from the use of asphyxiating or deleterious gases in war, if not for some time before, at least at its outbreak in the summer of 1914. I have, of course, no precise knowledge of the intentions of the German military authorities, but it was not unreasonable to surmise that these authorities, who deliberately intended to violate the treaty with Belgium, would not hesitate—as, indeed, the sequel showed—to disregard their promise under the Hague Convention if and when it suited their purpose to do so.

As regards their intentions, Field-Marshal Lord French, in his dispatch after the first German gas attack, with which Prof. Haber was concerned, wrote: "The brain-power and thought which has evidently been at work before this unworthy method of making war reached the pitch of efficiency which has been demonstrated in its practice shows that the Germans must have harboured these designs for a long time."

"It is an arresting thought," says Major Lefebure, "that even as early as 1887 Prof. Baeyer, the renowned organic chemist of Munich, in his lectures to advanced students, included a reference to the military value of these compounds"—*i.e.* to substances intended to produce temporary blindness.

Prof. Haber, it will be observed, does not explicitly deny the accuracy of the statements made by the "neutral," as quoted by Major Lefebure. Indeed, the account is too definite and specific to be set aside by irrelevancies. It is probably true that "everyone in Germany imagined that the course of the war would be a succession of rapid marches and great pitched battles." Some people on this side of the Channel were of a different opinion. But even the vain imaginings of "everyone in Germany" were not necessarily inconsistent with the use of poison gas. It was used on the Eastern front by the Germans when there was little or no question of trench warfare or wire entanglements. Prof. Haber states that he never met a single person who previous to the war or during the first two months of its course had conceived the idea of providing the army with gas. "The question of gas as a means of warfare did not begin to engage our attention until the first three months of war had passed."

The first gas attack was launched in April, 1915, so that on Geheimrat Haber's own showing this method of conducting war was engaging attention at least six months before it was used. After all, the essential point is that it was used, and first used, by the Germans, and in flagrant contravention of a solemn promise given to the world; at what precise

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period they decided to be false to their obligation is a matter of secondary moment.

The implication that the Allies were contemplating the use of poison gas as early as September, 1914, is so notoriously at variance with truth that it scarcely needs serious refutation. If Geheimrat Haber could only have been present when Lord Kitchener made his dignified protest in the House of Lords, or have been a personal witness of the wave of indignation and disgust which swept over the country at the Germans' breach of the rules of war, he would not have penned his statement. We were made aware of the rumours that were being spread through the German Press, but no credence was attached to them in this country. The German Empire, even to the last, had its "reptile Press" as in the days of the Iron Chancellor. The author of the Ems telegram was an adept in the art of circulating false rumours and misleading statements, and there were those who sought to better even his example during the fateful and, for the Germans, disastrous years of the war. Even now Germany does not know half the truth.

I agree with Prof. Haber that in war-time men are apt to think otherwise than they do in peace, and this is, doubtless, particularly true of his countrymen. Their ethical standard, apparently, varies with the two conditions—which seems a sufficient reason why they should abstain from war. Those who use poison gas are not "bonnie fechters."

The menace of the continued use of poison gas in warfare is a disgrace to civilised humanity. That menace really rests with Germany. If she would undertake for the future to be true to her obligation under the Hague Convention other nations would willingly follow her example.

They were reluctantly compelled to follow it in consequence of her action at the Battle of Ypres. They would far more promptly follow her lead if she announced her intention to discontinue the practice and gave the world a sufficient assurance of good faith.

Prof. Haber could render no greater service to civilisation and humanity than to use his great influence and pre-eminent position as a man of science in inducing his fellow-countrymen to remove what is a stigma upon their *Kultur*.

T. E. Thorpe.

Some Problems in Evolution.

I am quite as averse from "wasting time in endless and futile controversy" as Prof. Goodrich, but I cannot help thinking that so long as he and Sir Archdall Reid refuse to admit what seem to most biologists obviously true statements their arguments must be answered. Prof. Goodrich states in Nature of December 22 last that there is no contradiction between his proposition that characters, whether new or ancient, may be inherited provided they are possessed by both parents, and my reply that a character may be inherited when it is apparent only in one parent or in neither. If we omit what is common to both of these propositions it follows that in Prof. Goodrich's opinion there is no difference between "both parents" on one hand and "one or neither parent" on the other.

Prof. Goodrich complains that I do not distinguish between the variation and the resulting character. In his presidential address he maintained that the word "variation" should mean the extent or degree of difference between individuals, not a new character or assemblage of characters, such as a colour or spot on a butterfly's wing, but a difference which can be measured or estimated. "We shall then find," he writes, "that a variation is either due to some change in the complex of germinal factors or to some change in the complex of effective environmental stimuli." Here Prof. Goodrich and I are in complete agreement. Where, then, is any difference of opinion or room for controversy? The difficulty reappears in the next paragraph of Prof. Goodrich's letter, in which he states that he agrees with Sir Archdall Reid that there are two kinds of variation but only one kind of character.

Although I have honestly tried to do so, I fail to see any meaning in this statement. After all, words and terms are seldom completely satisfactory expressions of what we mean; they obtain their meaning by actual or implied reference to facts of observation. I have lately been in the habit of using the term "character" more frequently than the term "variation," because the latter is often ambiguous, but I know of no such difference of meaning between the two terms as is assumed by Prof. Goodrich and Sir Archdall Reid. How can we define characters satisfactorily except by comparison—that is, as differences between one individual, or one type, and another? For example, the rose comb is a character of certain fowls as compared with the single comb. What do we gain by asserting that the difference between the rose comb and the single is a variation, but the peculiarity of the rose comb is a character? The only questions of importance are the origin and the heredity of the rose comb. We understand one another when we say that the rose comb is inherited. It is quite superfluous to insist, as Sir Archdall Reid does, that characters are not transmitted, but only predispositions. No one supposes at the present day that the fowl's egg or spermatozoon has a rose comb, but we know that there may be something, whether we call it determinant, factor, or gene, in egg or sperm which causes the rose comb to develop in the resulting organism. And yet Sir Archdall Reid argues as though it were a remarkable discovery that characters are not present as such in the fertilised ovum from which an organism develops—an idea that has been obsolete since the Middle Ages. Therefore, he asserts, there is only one kind of character, but there are two kinds of variation. The hoof of a new-born foal has developed without any external mechanical stimulus; when I practise rowing for some time I develop epidermic corns on the palms of my hands. According to Sir Archdall Reid, these are characters of the same kind, equally innate, acquired, and inherited. Yet he has himself insisted on the distinction between characters developed under the "stimulus of nutrition" and those developed under the "stimulus of use," the same distinction, with his own peculiar misuse of the word *stimulus*, which is generally recognised by biologists.

Avoiding altogether the use of the terms "variation" and "character," we may congratulate ourselves that there is agreement on the proposition that a difference of form or structure may be due either to a difference of germinal factors or to a difference in effective environmental stimuli. And then we can get on with the investigation of the problem of the relation to evolution of these structural differences.

But, as I have endeavoured to show elsewhere, among those characters which are more or less completely hereditary there are two kinds, in a vast number of cases definitely distinguishable: the adaptive characters on one hand and the non-adaptive on the other. The adaptive characters exhibit a definite relation to habits and external conditions, and, as a rule, they exhibit recapitulation in development. The

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